SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

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FRIDAY, NOVEMBER 6, 1903.

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ADDRESS OF THE PRESIDENT OF THE SECTION OF EDUCATION OF THE BRITISH ASSOCIATION.

THE section over which I have the honor to preside deals with every branch of education. It is manifest that in an address your president can not deal with all of them, and it remained for me to choose one on which I might remark with advantage. As my official work during the last thirty-three years has been connected with education in science, I think I can not do better than take as my subject the action that the state has taken in encouraging this form of education, and to show that through such action there has been a development of scientific instruction amongst the artisan population and in secondary day schools. The development may not indeed have been to the extent hoped for, but it yet remains that solid progress has been made.

I have chosen the subject deliberately, as I find that there are very few of those who have the interests of education strongly at heart, or who freely criticize those who have borne the burden of the past, that have any knowledge of the trials and difficulties (some of its own creating, but others forced on it by public opinion) which the state, as represented by the now defunct Science and Art Department, had to contend with in its unceasing missionary

efforts in the cause of scientific instruction. I shall not attempt to do more than show that whatever its defect may have been in tact, whatever its shortcomings in method, that department still deserved well of the country for the work that it did in regard to the fostering of scientific instruction in the country at large.

As far back as 1852 the government of the day, influenced very largely by the Prince Consort, realized that it had an educational duty to perform to the industrial classes. Whether it was influenced by philanthropic motives or from the evidence before it that if Great Britain was to maintain its commercial and industrial supremacy scientific instruction was a necessity, it matters little. The fact remains that it determined that the industrial classes should have an opportunity of acquiring that particular kind of knowledge which would be of service to them as craftsmen. In this year 1852 the Speech from the Throne contained these words: "The advancement of fine arts and practical science will be readily recognized by you as worthy of a great and enlightened nation. I have directed that a comprehensive scheme shall be laid before you, having in view the promotion of those objects towards which I invite your aid and cooperation."

It is somewhat remarkable that the then ministry, of which Lord Derby was the chief and Mr. Disraeli the chancellor of the exchequer, did not survive to promulgate the scheme, which proposed theoretical rather than practical science, but that their successors, under Lord Aberdeen, issued it and commenced to carry it into effect. In 1853 the Department of Science and Art was established under the direction of Mr. Cole. Since 1835 so-called schools of design had been in being. These came under the new department, and it was determined to establish science classes for instruction in science, Dr. Lyon Playfair, the well-known chemist, being charged with the duty. Playfair resigned in 1858, and in 1859 Mr. Cole induced a young engineer officer, Lieut. Donnelly, to undertake the inspection and organization of science instruction throughout the country. It was through this officer's untiring energy and zeal that the classes in science flourished and were added to at this early stage of the new department's history. The same energy was displayed by Donnelly during the whole of his long career in the service of the state, and I feel that it was fortunate for myself to have served so many years as I did under one to whom the country at large owes a deep debt of gratitude.

Not long ago he passed away from us, and there will be no more lasting memorial to him than that which he himself erected during his lifetime in the fostering of that form of education which is of such vital importance to the national well-being.

To revert to history, I may record that the first science examinations conducted by the state took place in May, 1861, and, the system of grants being made on the results of examination having been authorized, the magnificent sum of 1,300*l.* was spent on this occasion on the instruction of 650 candidates, that number having been examined. Thus early was the system of examination commenced in the department's career, and the method of payments on the results of these examinations stereotyped for many years to come. There is reason to believe that the educational experts of that day considered that both were essential and of educational value, a value which has since been seriously discounted. Employers of labor in this country were not too quick in discerning the advantages that must ultimately ensue from this class of education if properly carried out and encouraged. Theoretically they gave en-

couragement, but practically very little, and this survives to some extent even to the present day. Some of the foremost employers, however, gave material encouragement to the formation of classes, insisting on their employees attending evening instruction; but conspicuous above all was Mr. Whitworth, who, in 1868, placed in the hands of the department the sum of 100,000*l*., to be devoted to the creation of scholarships, which were to be awarded at the annual May examinations. The proviso made by him was that all competitors were to have had experience in practical work in an engineering establishment. Such candidates, it was evident, must have found out their own weakness in education, and, by working in science classes, could make up their deficiencies, and the award of these scholarships would enable them to study further. Sir J. Whitworth was far-seeing and almost lived before his age, but the benefits that he has conferred, not only on individuals, but on science and industries, by his generosity will make his name to be remembered for generations to come. To have been a Whitworth scholar gives an *entrée* into various government and engineering posts, and we have in the front rank of science men who have held these scholarships and whose names stand prominent in the development of engineering.

Incidentally, I may say that no country but this, for very many years, considered that instruction in science for the artisan was a large factor in maintaining and developing industry. The educational interests of the employer and the foreman were, in some countries, well provided for, but the mechanic was merely a hand, and a 'hand' trained in merely practical work he was to remain. He could not aspire to rise beyond. We may congratulate ourselves that such a 'caste' system does not exist amongst ourselves.

For the first twenty-five years of the Department of Science and Art the grants given by parliament for science instruction were distributed almost entirely amongst those who were officially supposed to belong to the industrial classes, and no encouragement was offered to any higher class in the social scale.

It would take me too long to show that at first the industrial classes were very shy of seizing on the advantages offered them. Suffice it to say that they had to be bribed by the offer of prizes and certificates of success to attend instruction, and it was not for several years that the evening classes got acclimatized and became popular.

The evening instruction was then largely attended by adults. That this was the case may be judged by the fact that the average age of candidates who obtained successes in advanced chemistry was about twenty-five and in elementary chemistry about twenty-one. I have alluded to the apathy displayed by employers and by the artisans in the early days of the Department of Science and Art. The causes which dispelled it in both employers and employed, in regard to science instruction, will be found in the following extract from a report by the Department of Science and Art:—

> The Paris Exhibition (1867) caused the work of this country to be brought into close comparison with that of the rest of the continent, and in many points both of manufacture and of skilled labor it was found that England did not stand in such a good position as she had done a few years back. Dr. Playfair, in a letter to the *Times*, drew attention to this, attributing much if not all the evil to the deficiency of our technical education among the artisan class. The substance of this letter was taken up by many persons of influence during the autumnal recess, and it led to a sort of educational panic, the cry for technical education becoming quite the absorbing topic among all circles and forming a considerable portion of the contents of all periodicals. Meetings were convened and addresses delivered all over the country, and the question was so much ventilated that im-

portant changes were anticipated in the educational arrangements of the country during the coming session of Parliament, which unfortunately were put off on account of the debates on the Reform Bill of 1868.

The agitation necessarily brought forward the work of the Science Division of the Science and Art Department, and it is not a little remarkable how completely the system which had been growing up since 1860 seemed to meet all the requirements of the case, and at the same time how few persons had any idea of its provisions in spite of all that had been done to spread a knowledge of the scheme.

There can be no doubt, however, but that this six years' work had silently, though materially, effected a change in the general tone of feeling on the subject of scientific education, and had been the means of preparing the country for the 1867 agitation. The different feeling among the working-classes on the subject is forcibly shown in the annual report of the Science and Art Department. From this it appears that in 1860 a pupil in one of the science classes in Manchester, a town usually looked upon as in advance of others, could hardly continue his attendance at the class owing to the taunts of, and ill-treatment by, his companions. Nevertheless, in the autumn of this year, 1867, hardly enough could be said or done to satisfy the desire for science classes being formed for those very persons who, but six years before, had considered attendance at a government science school as almost against the rules of their trade.

Such was the account of 1867 given by Mr. G. C. T. Bartley (now Sir G. Bartley, M.P.). The plan adopted by the Science and Art Department for encouraging instruction in science was perhaps the best that could be devised at the time, though we now know that it was capable of improvement. It may be mentioned that an improvement in it was made the next year by the introduction of a very large system of scholarships, scholarships which have enabled the possessors in some instances to continue their studies at universities, and several distinguished men owe their positions to this aid. It was in this same year that Mr. Whitworth established his scholarships, as before described.

I have endeavored to give a brief *résumé* of what was done during the first fifteen years of the existence of the Science and Art Department, and it continued to expand its operations after 1868 on the same lines for another ten years. In 1876 your president became connected with the department as a science inspector. I am sure the section will forgive me if I am somewhat personal for a few moments. During the previous eight years I had had the honor of being a teacher of some branches of physical science at the School of Military Engineering, and my own training was such that I had formed a very definite opinion as to how science instruction should be imparted, both to those who had a good general education and also to those who had not. The method was the same in both cases: it should be taught practically. I may say that though I had not myself had the advantage of being taught it at school, I had learned all the science I knew practically, and I entered the department fully impressed with this view. Whenever possible I have until the present time endeavored to impress this view on all who were interested in the work of the department. Much of the science that was taught in state-supported classes was largely book work and cram, and the theoretical instruction as a rule was unillustrated by experiment. This was undoubtedly due to the system of payments being based on success at the examinations. I must here say that there were honorable exceptions to this procedure. There were teachers, then as now, who knew the subjects they taught, and who were inspired by a genuine love of their calling. I can in my mind's eye recall many such, some of whom have joined the majority and others who are still at work and as successful now as then in rousing the enthusiasm of their students.

I am not one of those who think, as some

do, that cramming is entirely pernicious. A good deal of what used to be taught at public schools in my days was cram. It served its purpose at the time in sharpening the memory, and was a useful exercise, and it did not much matter if in after years much of it was forgotten. If the cramming is in science, a few facts called back to mind in after life are better than never having had the chance at all. In fact, as the faded beauty replied to the born plain friend, it is better to be one of the 'have beens' than a 'never wasn't.'

It was determined to make a vigorous onslaught against teaching that was unillustrated by experiment and to encourage practical teaching as far as could be done. Proper apparatus for illustrating lectures was insisted upon, and, with aid from the department, was eventually provided, though in some instances several years' pressure had to be exercised before it was obtained. I am bound to say that in many instances after it had been procured a surprise visit by the inspector during the hours of instruction often found that the lecture table was free from all encumbrance, and that the dust of weeks was upon the apparatus that should have been in use. This was sometimes due to the inability of the teacher to use the apparatus rather than to a wish to disregard the rules laid down by the department; but usually it was due to the fact that the teacher found cram paid best. I should like to say here that this state of things does not exist at the present time, and that the training of science teachers by the Royal College of Science and by other institutions has completely broken down the excuses that were often offered at that time.

The first grants for practical teaching were paid for chemistry. The practical work had to be carried out in properly fitted laboratories. There were not half-a-dozen at the time which really answered our purpose, and one of the earliest pieces of work on which I was engaged was in assisting to get out plans for laboratory fittings. These were very similar to those which I had designed for the School of Military Engineering several years before. Thanks to the Education Act of 1870 (I speak thankfully of the work that some of the important school boards have done in the past in taking an enlightened view of science instruction), there were some localities where the idea of fitting up laboratories was received with favor, and it was not long before several old ones were refitted, in which instruction to adults was given, and new ones established in board schools for the benefit of the sixth standard children. At that time an inspector's, like the policeman's, lot was not a happy one. We had to refuse to pass laboratories which did not fulfil conditions, though we left very few 'hard cases.'

Until after the passing of the Technical Instruction Act in 1887 the department aided schools in the purchase of the fittings of laboratories (both chemical and others), and year after year this help, which stimulated local effort, caused large numbers of new laboratories to be added to the recognized list. After six or seven years we had a hundred or more laboratories at work of what I may call 'sealed-pattern efficiency.' I am not very partial to sealed patterns, but they are useful at times, for they tell people what is the least that is expected from them. The pattern was not without its defects; but laboratories, like other matters, follow the law of evolution, and the more recently fitted ones show that the experience gained whilst teaching or being taught in a sealed-pattern type has led to marked improvements. Personally I am of opinion that only necessaries should be required, and I rebel against luxuries; for a student trained by means of the latter will, as a rule, in after life

fail to meet with anything beyond the mere essentials for carrying on his scientific work.

The sealed pattern is practically in abeyance, though it can be trotted out as a bogey, and any properly equipped laboratory is recognized so long as it meets the absolute necessities of instruction.

The half-dozen chemical laboratories which existed in 1877 have now expanded to 349 physical and 774 chemical laboratories. These are spread over all parts of England. I leave out Scotland and Ireland, as the science teaching is no longer under the English board of education.

It is only fair to say that many of this large number of laboratories are at present in secondary schools, regarding which I shall have to speak more at length. But the fact remains that in twenty-seven years there has been such a growth of practical science teaching that some 1120 laboratories have come into being. My predecessor in the chair likes to call laboratories 'workshops.' I have no objection, but the reverse; for the word 'laboratory,' like 'research,' sounds too magnificent for what is really meant, and all education should more or less be carried out in workshops.

The increase is as satisfactory as it is remarkable. It was only possible to increase the numbers in early days by gentle pressure and prophesying smooth things which, happily, did eventually come to pass. In latter days the increase has been almost automatic. The Technical Instruction Act has called into being technical instruction committees who in many cases have taken up science instruction in their districts in earnest. They, too, have had public money to allocate, and not a little has gone in the encouragement of practical education. It may, however, be remarked that had it not been for the preliminary work that had been done by the Science and Art Department it is more than probable that the Technical Instruction Act of 1887 would never have seen the light.

A reference must now be made to the removal of what any one will see was a great bar to the spread of sound instruction in every class of school where science was taught. So long as the student's success in examination was the test which regulated the amount of the grant paid by the state, so long was it impossible to insist on all-round practical instruction. It was impracticable to hold practical examinations for tens of thousands of students in some twenty different subjects of science. The practical examination in chemistry told its tale of difficulties. It was only when the Duke of Devonshire and Sir John Gorst in 1898 substituted for the old scheme of payments payment for attendance, and in a large measure substituted inspection for examination, that the department could still further press for practical instruction. For all elementary instruction the test of outside examination does more harm than good, and any examination in the work done by elementary students should be carried out by the teacher, and should be made on the absolute course that has been given. It seems to be useless or worse that an examination should cover more than this. Instruction in a set syllabus which for an outside examination has to be covered spoils the teaching and takes away the liberty of method which a good teacher should enjoy. The literary work involved in answering questions, for an outside examiner, is also against the elementary student's success, and can not be equal to that which may properly be expected from him a couple of years later.

Advanced instruction appears to be on a different footing. The student in advanced science must have gradually obtained a knowledge of the elementary portions of the subject, and it is not too much to ask

him beyond the inspection of his work to express himself in decent English and to submit to examination from the outside; but even here the payment for such instruction should be by an attendance grant tempered in some degree by the results of examination, since examiners are not always to be trusted.

The attendance grant was not viewed by some with great favor at first, and protests were received against its adoption, a favorite complaint being that it was sure to entail a loss of grant. One became suspicious that some of those who protested were aware that the last bulwark which defended the earning of grants by cram was being removed, and that inspection might prove more irksome than examination. This is past history now, and the new system works as smoothly as the old and with not more complaints than are to be always expected.

As I have said, grants were for very many years supposed to be confined to aiding the instruction of the industrial classes, but this limitation was more nominal than real. It might probably be imagined that it was no very difficult task to distinguish an artisan and his children from students who belonged to the middle classes. This was not the case, however. Children belonging to the industrial class were, on joining a science class, obliged to state the occupation of the father, and it was no uncommon thing for fathers to be given brevet-rank by their children. Thus, a brick-layer's son would describe his father as a 'builder,' which, if true, ought to have brought him into the ranks of the middle class. These unauthorized promotions were one of the difficulties the inspector had to face when judging as to the status of the parents. This difficulty was largely met by a rule that all those who attended evening classes were supposed to be of the industrial class; but as day classes increased the numbers of those who by no possibility could be of the artisan class also increased, and it became a very invidious duty of the inspector to put M.C. (Middle Class) against the names of many. It was determined by superior authority that only those students or their parents who could claim exemption from income-tax should be reckoned as coming within the category of industrial students. In early days the qualification for abatement on income-tax was a much lower figure than it is to-day, and almost each succeeding chancellor of the exchequer has raised the figure of the income on which the abatement could be claimed. To-day it is, I believe, 700*l.* a year, bringing the official definition as to membership of the industrial classes to an absurdity. It became evident to the official mind, which some people are good enough to say works but slowly, that the definition must be amended or the limitation abolished. The progress of events happily made the abolition the better plan, and was the means of allowing inroads of science instruction to be made into secondary day schools.

The history of these inroads I shall now give. Instruction given in so-called organized science schools was originally aided by the department by means of a small capitation grant. These schools were supposed to give an organized course of science instruction, and the successes at examination determined the payment. They were not satisfactory as at first constituted, and they so dwindled away in numbers that in 1890 only some one or two were left. A small increase in capitation grant in 1892 revived some of them, and a fair number existed in the following year. There was no doubt, however, that the conditions under which they existed were most unfavorable for a sound education, which ought not only to include science but also literary instruction. The latter was, in many schools,

wholly neglected, owing to the fact that the grants earned depended on the results of examination, and so all the school time was devoted to grant earning.

Mr. Acland, at this time minister for education, was made aware of this neglect to give a good general education, and as I was at that time responsible for science instruction I was directed to draw up a scheme for reorganizing these schools and forcing a general as well as scientific education to be carried out. Baldly the scheme abolished almost entirely* payments on results of examination, and the rate of grant depended on inspection and attendance. Further, a certain minimum number of hours had to be given to literary subjects, and another minimum to science instruction, a great deal of it being practical and having to be carried out in the 'workshop.' The payments for science instruction were to be withheld unless the inspector was satisfied that the literary part of the education was given satisfactorily.

The scheme was accepted and promulgated whilst the Royal Commission on Secondary Education was sitting, and, if I may be allowed to say so, Mr. Acland's tenure of office would be long remembered for this innovation alone, since in it he took a wide departure from the traditional methods of the department and created a class of secondary school which differed totally from those then existing. Needless to say the scheme was not received with favor on all sides, more especially by those who thought that serious damage would be done to secondary schools by the competition from this new development of secondary education. I am not ashamed to say that the disfavor shown on some sides made me rejoice, as it indicated that a move had been made in the right direction. At first it was principally the higher-grade board schools that came under the scheme, and in the first year there were twenty-four of them at work. This type of school gradually increased until about seventy of them, and chiefly of a most efficient character, were recognized in 1900. Their further increase was only arrested by the Cockerton judgment, now so well known that I need only name it. But here we come to a most interesting development. State aid, as already said, was at first limited to the instruction of the industrial classes, but no limitation as to the status of the pupil was made in this new scheme for the schools of science, and logically this freedom was extended in 1897 to all instruction aided by the department—the date when all limitation as to the status of the pupil was abolished, the only limitation being the status of the school itself. Thus, if a flourishing public school, charging high fees for tuition, were to apply to participate in the grant voted by parliament, it may be presumed, it would have to be refused. The abolition of the restriction as to the status of the pupils left it open to poorly endowed secondary grammar schools to come under the new scheme. To a good many the additional income to be derived from the grant meant continuing their existence as efficient, and for this reason, and often, I fear, for this reason alone, some claimed recognition as eligible.

Such is an outline history of the invasion of science instruction into certain secondary schools—an invasion which ought to be of great national service. In my view no general education is complete without a knowledge of those simple truths of science which speak to every one, but usually pass unheeded day by day. The expansion of the reasoning and observational powers of every child is as material to sound education as is the exercise of the memory or the acquisition of some smattering of a language. I am not going into the

* Within the next four years they will entirely cease.

question of curricula in schools, as I hope, regarding them, we shall have a full discussion. But of this I am sure, that no curriculum will be adequate which does not include practical instruction in the elementary truths of science. The president of the Royal Society, in his last annual address, alluded to the medieval education that was being given in a vast number of secondary schools. Those who planned the system of education of those times deserve infinite credit for including all that it was possible to include. Had there been a development of science in those days, one must believe that with the far-seeing wisdom they then displayed they would have included that which it is the desire of all modern educationists to include. Observational and experimental science would have assuredly found a place in the system.

One, however, can not help being struck by the broadening of views in regard to modern education that has taken place in the minds of many who were certainly not friendly to its development. Perhaps in the Bishop of Hereford, when headmaster of Clinton, we have the most remarkable early example of breadth of view, which he carried out in a practical manner, surrounding himself with many of the ablest teachers of science of the day. There are other headmasters who, though trained on the classical side, have had the prescience to follow in his footsteps, and of free will; but others there are who have neither the desire nor the intention, if not compelled to do so, to move in the direction which modern necessities indicate, is essential for national progress. I am inclined to think that the movement in favor of modernizing education has been very largely quickened by the establishment of schools of science in connection with endowed schools and the desire for their foundation by the technical instruction committees, who had the whisky money at their disposal, and who often more than supplemented the parliamentary grants which these schools were able to earn. It was the circumstance that the new scheme was issued when many endowed schools were in low water that made it as successful as it has been.

The number of schools of science increased so rapidly that it appeared there might be a danger of too many of this type being started on sufficient educational grounds. Science instruction was carried in them to such an advanced point and so many hours of the week were spent on it that they became in some degree specialized schools. At least eight hours a week had to be devoted to science, ten to literary instruction, and five to mathematics—any further time available could be spent on any section that was considered desirable. For some pupils the time devoted to science is barely enough, but for others who intend to follow careers in which the literary section should predominate it appeared that some curtailment of hours in the science section might be usefully allowed, and it became a question how far such instruction might be shortened without impairing its soundness. After much anxious thought it was considered that four hours per week, besides mathematics, was the very least time that ought to be devoted to such instruction, and that the latter part of it should be practical work. A scheme embodying this modification was approved by the lord president and the vice-president whilst I was principal assistant secretary for secondary education, and smaller grants than those for schools of science were authorized in 1901 for those schools which were prepared to adopt it. By the scheme instruction has to be given only in such subjects and to such an extent as is really necessary to form part of that general education of ordinary students who might not have to follow industrial pursuits. This modified and shortened course has met with

unqualified success. Some 127 schools came under the scheme the first year, and I gather that there will be a considerable increase in numbers in the future. The establishment of schools of science and of these schools may be considered to be a great step taken in getting practical instruction in natural knowledge introduced into secondary schools. The leaven has been placed in some 300 of them, and we may expect that all schools which may be eligible for state aid will gradually adopt one scheme or the other. Though it is said that there is nothing in a name, I am a little doubtful as to whether the earmarking of science education as distinct from secondary education is not somewhat of a mistake at the present day. For my own part, I should like to think that the days have passed when such an earmarking was necessary or advisable. The science to be taught in secondary schools should be part and parcel of the secondary education, and it would be just as proper to talk of Latin and Greek instruction apart from secondary education as it is to talk of science instruction. One of the causes of the unpopularity of the Science and Art Department was its too distinctive name. At the same time it would be most unwise at the present time, when the new education committees are learning their work and looking to the central authority for a lead, for the state to alter the conditions on which it makes its grants to these schools. It will require at least a generation to pass before modernized education will be free from assault. If science instruction is not safeguarded for some time to come it runs a good chance of disappearing or being neglected in a good many schools. As to the schools which have no financial difficulties, it is hard to say what lines they may follow. Tradition may be too strong in them to allow any material change in their course of study. If it be true that the modern side of many a public school is made a refuge for the 'incapables,' and is considered inferior to the classical side, as some say is the case, such a side is practically useless in representing modern education in its proper light. Again, one at least of the ancient universities has not shown much sympathy with modern ideas, and so long as she is content to receive her students ignorant of all else but what has been called medieval lore, so long will the schools which feed her have no great inclination to change their educational schemes.

If we would only make the universities set the fashion the public schools would be bound to follow. The universities say that it is for the public schools to say what they want, and *vice versa,* and so neither one nor the other changes. It appears to me that we must look to the modern universities to lead the movement in favor of that kind of education which is best fitted for the after life of the large majority of the people of this country. If for no other reason, we must for this one hail the creation of two more universities where the localities will be able to impress on the authorities their needs. The large majority of those whose views I share in this matter are not opposed to or distrust the good effects of those parts of education which date from ancient times. The great men who have come under their sway are living proofs that they can be effective now as they have been in times past, but we look to the production of greater men by the removal of the limitations which tradition sets. I myself gratefully acknowledge what the public school at which I had my early education did for me, but I think my gratitude would be more intense had I been given some small elementary instruction in that natural knowledge which has had to be picked up here and there in after life.

There is one type of college which I have not alluded to before, and that is the technical institutes. These have been fostered by the localities in which they are situated, and been largely supported by the whisky money, supplemented by government aid. I am glad to see that in the last regulations of the board of education these colleges will receive grants for higher scientific instruction, and I have no doubt that in the near future such institutions and schools of science will receive a block grant, which will give them even still greater freedom than they now enjoy. These are colleges to which students from secondary schools will gradually find their way, where they wish for higher education of a type different from that to be gained at a university.

I have endeavored to give a brief historical sketch of what the state has done in helping forward instruction in natural knowledge amongst the industrial classes, adults and children, and how gradually its financial aid has been extended to secondary schools. I have also endeavored to indicate the steps by which practical instruction has been fostered by it. I have done this because I am confident that ninety-nine educationists out of every hundred have but little idea what the state has been doing for the last fifty years. Some connected with secondary schools—I have personal knowledge—were until lately ignorant that the state had offered advantage to them of a financial nature. I may say that the work of the late Science and Art Department was largely a missionary work. It was abused, sometimes rightly but more often wrongly, for this very work, and it had more abusers at one time probably than any other government department. Even friends to the movement of modernizing education found fault with it as antiquated and slow, but I can assure you that no greater mistake can be made in pressing forward any movement than by any hurried change of front or by endeavoring to push forward matters too rapidly. In the first place, the treasury naturally views untried changes with suspicion, and this fact has to be dealt with more particularly when there is no great expression of public opinion to reckon with. At the same time it can not be stated too strongly that the treasury has in recent years dealt in a friendly and enlightened spirit with all matters which could affect the spread of science. Again, there is a hostility to great and rapid changes in the minds of those whom such changes affect.

The policy must always be to progress as much as is possible without rousing too great an opposition from any quarter, and I think it will be seen that the progress made during the last twenty-five years has, by the various annual increments, been perhaps more than could have been hoped for, and gives a promise for even more rapid advance in the future.

As an appendix to this address I have given a brief epitome of the increases in students, in schools, in laboratories, and in grants which have taken place since 1861. If to the last be added the amount spent out of the whisky money an additional half million may be reckoned.

It will be seen that the progress made has been gradual, but satisfactory, and that, if we showed some of the results graphically, weighed according to the circumstances of their date, and dared make an extrapolation curve of future results, we should have a complete justification for prophesying hopefully.

The question of the supply of science teachers has already been referred to. My remarks I should like to supplement by saying that in the greater number of schools teachers are to be found who have been trained at the Royal College of Science, and mostly at public expense—some

through scholarships gained by competition and some through training selected teachers. The success of the movement for the introduction of science instruction in schools depended on the proper supply of teachers, and even now the demand for men possessing the highest teaching qualifications in science is greater than the supply. It may be said, I think, that our science teachers from the college have one special qualification, and that is, that besides the knowledge of science, practical and theoretical, that they have acquired, they have lived in an atmosphere of what is called research, and which might be called original investigation. Professors, assistants and students alike are impregnated with it, and when the teacher so trained takes up his duties in his school he still retains the 'reek' of it. True instruction in science should, as I have before said, be practical, and practical instruction should certainly include original inquiry into matters old or new. The teacher who retains the 'reek' is the teacher who will prove most successful. It will thus be seen that the state had the task before it, not only of introducing instruction in science, but of training teachers to give such instruction. This problem is the same as now exists in Ireland, and the experience gained in England can not but be of the greatest use to those at the head of Irish technical education.

Before concluding there is one subject that I must lightly touch upon, and that is the supply of teachers other than science teachers. The Education Act of 1870 gave the power to elementary schools to train pupil teachers, who in the process of time would become teachers, either by entering into a training college by means of a King's Scholarship or, less satisfactorily, by examination. In large towns the need of a proper training for pupil teachers has been felt, and gradually pupil teacher centers were established, principally by school boards, where the training could be carried out more or less completely; but in the rural districts and smaller towns the pupil teacher has had to be more or less self-taught, and except in rare cases 'self-taught' means badly taught. The training college authorities make no secret of the fact that one of the two years during which the training of the teacher is carried out has to be devoted more or less to instructing the pupils in subjects they ought to have been taught before they entered the college. Thus all the essential and special instruction which is given has to be practically shortened, and the teacher leaves the college with less training than he should have.

The new Education Act has put it in the power of the educational authorities to rectify the defects in the training of pupil teachers. It is much to be hoped that councils will separately or in combination either form special centers for the training of all pupil teachers or else give scholarships (perhaps aided by the state) to them, to be held at some secondary school receiving the grant for science and recognized by the board of education as efficient. The latter plan is one which commends itself, as it ensures that the student shall associate with others who are not preparing for the same calling in life, and will prevent that narrowness of mind which is inevitable where years are spent in the one atmosphere of pedagogy. The non-residential training college, where the training of the teacher is carried on at some university college, is an attempt to give breadth of view to him, but if attempted in the earliest years of a teacher's career it will be even more successful. All teaching requires to be improved, and the first step to take in this direction is to educate the pupil teacher from his earliest day's appointment, for his influence in after years

will not only be felt in that elementary, but will also penetrate into secondary education. In regard to the additions which are required in elementary education, and which require the proper training of the pupil teacher, I must refer you to a report which will be presented to the section. The task of training pupil teachers is one which requires the earnest and undivided thought of the new education committees.

In the earnest address given by my predecessor in this chair he brought forward the shortcomings of secondary education and of the requirements for a military career in a trenchant manner and with an ability which I can not emulate. With much of what he said I agree heartily, but I can not forget that, after all, the details of education are to some extent matters of opinion, though the main features are not. We must be content to see advances made in the directions on which the majority of men and women educational experts are agreed. Great strides have already been made in educating the public both in methods and subjects, but a good deal more remains to be done.

It may be expected, for instance, that the registration of teachers will lead to increased efficiency in secondary schools, and that the would-be teacher, fresh from college, will not get his training by practising on the unfortunate children he may be told off to teach. It may also be expected that such increased efficiency will have to be vouched for by the thorough inspection which is now made under the board of education act, by the board, by a university, or by some such recognized body. It again may be expected that parents will gradually waken up to the meaning of the teacher's register and the value of inspection, and that those schools will flourish best which can show that they too appreciate the advantages of each.

I have to crave pardon for having failed to give an address which is in any way sensational. I have thought it better to review what has been done in the past within my own knowledge, and with this in my mind I can not but prophesy that the future is more than hopeful, now that the public is beginning to be educated in education. It will demand, and its wants will be supplied.

WILLIAM DE W. ABNEY.

APPENDIX.

Number of Schools of Science and their Grants.

Year.	Higher Grade Schools.	Endowed Secondary Schools.	Technical Institutes.	Total Schools.	Total Grants.
					£
1895	53	30	29	112	39,163
1898	69	50	49	168	98,849
1901	63	106	43	212	118,833
1903	50	119	57	226	Not yet known.

Number of Schools teaching Shortened Course of Science.

Year.	No.
1902	127
1903	184

Number of Laboratories Recognized.

Year.	Chemistry.	Metallurgy.	Physics.	Biology.	Mechanics.
1880	133	—	—	—	—
1900	669	37	219	17	4
1901	722	37	291	26	10
1902	758	39	320	34	14

Grants paid for Science Instruction.

Year.	Amount.	Year.	Amount.
	£		£
1860	709	1890	103,453
1870	20,118	1895	142,543
1875	42,474	1901	212,982
1880	40,229	1902	240,822
1885	63,364		

*THE LONGITUDE OF HONOLULU, VARIOUS DETERMINATIONS, 1555–1903.**

THE occasion for this article is the recent determination by the telegraphic method of the difference of longitude of San Francisco and Honolulu through the new Pacific cable by Messrs. Edwin Smith and Fremont Morse, of the Coast and Geodetic Survey.

The first signals over this portion of

* Read before the Philosophical Society of Washington, October 10, 1903.

the cable were sent on January 1, 1903. The cable was opened for public use on January 5, 1903. The first observations in the telegraphic determinations of longitude were secured on April 20.

The cable company cooperated with the government by aiding the observers in various ways, and by granting the free use of the cable during a short period each night for their work.

Between April 20 and June 13 eleven determinations of the difference of longitude were made, five with the observers in one position and six after they had exchanged places for the purpose of eliminating the effects of their relative personal equation. The total range of the eleven results was only 0.17 s. No result differed from the mean by as much as one tenth of a second.

This degree of accuracy corresponds to that usually attained in determinations of the difference of longitude of two places connected by a telegraph line overland. Until within a few years such a degree of accuracy had not been possible for determinations made through long cables. The increase in accuracy has apparently been due to improvements made in the recording apparatus used by the cable companies.

The difference of longitude of the transit piers in San Francisco and Honolulu, respectively, was found to be, from the field computation, 2 h. 21 m. 38.92 s.

A revision of the computation still remains to be made, but the changes in this result will be very small. The chances are even for and against the difference stated, being within 0.03 s. of the truth.

The longitude of San Francisco from Greenwich is fixed by four transatlantic determinations by the telegraphic method, 1866, 1870, 1872 and 1892, and by a complicated telegraphic longitude net stretched across the United States.

The longitude of the Transit of Venus pier at Honolulu, as fixed by the new determination from San Francisco, is 10 h. 31 m. 27.24 s. west of Greenwich. Taking into account all the uncertainties in the chain of observations between Greenwich and Honolulu, the chances are even that the value stated is correct within 0.06 s., and it is almost certain that it is not in error by as much as 0.2 s. In the latitude of Honolulu 0.06 s. of longitude corresponds to 85 feet, or 1 s. to 1,418 feet.

The observers, Messrs. Smith and Morse, are still engaged in determining the differences of longitude over the three other spans of the cable between Honolulu and Manila. A good determination of the difference Manila–Guam has already been secured. When their work is complete the longitude of Manila will have been determined telegraphically in both directions around the world from Greenwich, and the longitude girdle of the earth will be complete.

The following résumé of various determinations of the longitude of Honolulu is condensed from an account furnished by Mr. W. D. Alexander, formerly Surveyor-General of the Hawaiian Islands, and now an assistant in the Coast and Geodetic Survey. It is especially interesting as showing the comparison of various determinations of a large difference of longitude by a variety of methods and extending over three and a half centuries.

In the following tabular statement the new determination of the longitude of the Transit of Venus station, 10 h. 31 m. 27.2 s., is taken as being correct in deriving the errors of the various determinations. In cases in which the older determinations referred to some other point than the Transit of Venus station, they have been reduced to that station by using the relations in position which are now known.

	Longitude of Transit of Venus Pier, Honolulu.	Error of Longitude.
	h. m. s.	
John Gaetano, 1555, no chronometers, no log	9 23	—68m
Cook, 1785, observatory on Hawaii	10 31 45	+18'
Vancouver, 1790, based on Cook, various points	45 to 27	+18 to 0
Freycinet, 1819	26	— 1
Wilkes, 1840, based on Cook	35	+ 8
Lyman, 1845–1846, moon culminations, predicted places	15	—12
Fleuriais, 1868, 27 moon culminations, first computation	21.8	— 5.4
Fleuriais, 1868, second computation	25.9	— 1.3
Tupman, 1874, 700 culminations, 13 occultations, 540 zenith distances of moon, first computation	15	—12
Tupman, 1874, second computation	27.2	0.0
Tupman, 1874, combined with first computation of Fleuriais	26.3	— 0.9
Tupman, 1874, combined with second computation of Fleuriais	26.7	— 0.5
Tupman, 1875, 20 chronometers, one trip, Honolulu to San Francisco.	33.2	+ 6.0
Hawaiian Government Survey, 1884, chronometers, 2 round trips, Honolulu to San Francisco	25.8	— 1.4
Hawaiian Government, adopted in furnishing time	26	— 1.2
Hawaiian Government, adopted for mapping purposes	27.2	0.0

In the Spanish chart found by Lord Anson on board the galleon which he captured in 1743, a group of islands was laid down in the same latitude as the Hawaiian Islands, but 17° too far east.

The southernmost and largest island was named La Mesa, which seems to refer to Hawaii with its high tableland. North of it were La Desgraciada, probably Maui; and three small islands called Los Monjes, which may have been Kahoolawe, Lanai and Molokai.

This chart was published in the narrative of Lord Anson's voyage in 1749.

An official letter from the Spanish Hydrographical Department to the Hawaiian Government, dated Madrid, February 21, 1865, states that an ancient manuscript chart exists in the archives of that office, in which this group is laid down as in the chart of the Spanish galleon, with the name 'Islas de Mesa,' and a note declaring that they were discovered and named by Juan Gaetano in 1555.

The large error in the longitude of La Mesa, of 17°, more than 1,000 nautical miles, is not surprising when it is considered that chronometers were not yet dreamed of, that the Spanish navigators depended entirely on dead reckoning for their longitudes, that the use of the log for measuring the velocity of a ship was not known before 1607, and that the equatorial current would be effective in producing an error of the sign actually found. Thus La Pérouse, coming from California, found that the error in his dead reckoning caused by this current, when he arrived off Hawaii, amounted to 5° to the east, and Vancouver, coming from the south, found a similar error from the same cause, amounting to 5°14′.

When Captain Cook discovered Hawaii, in 1768, he was not aware that he had accidentally rediscovered La Mesa, and his successors at first retained both La Mesa and the Sandwich Islands on their charts, as may be seen in the atlas accompanying the early editions of Cook's 'Voyages.'

Seven years later, in 1785, two of Cook's officers, Portlock and Dixon, on their way to the northwest coast, as their crews were suffering from scurvy, headed their ships for the supposed position of La Mesa, sailed over it, and ran down the parallel of latitude till they arrived at Hawaii. A few days later La Pérouse, after searching in vain for La Mesa, did the same, and be-

came convinced of its identity with Hawaii.

The charts attached to the early editions of Cook's 'Voyages' placed Kealakekua Bay at 156°00′, or 18 s. too far west.

Although chronometers were put into use prior to 1714, as late as 1762 John Harrison won a reward of 20,000 pounds from the British government for having constructed a chronometer which, according to the test made, determined the difference of longitude of Portsmouth, England, and Jamaica within eighteen miles. The prize in question was offered in 1714, and it took 47 years to make sufficient improvements in chronometers to win it. Captain Cook's error of only 18 s., or four and one half miles in the longitude of Honolulu was, therefore, remarkably small for that time.

Captain Vancouver, in 1790, adopted the same longitude of Kealakekua as Captain Cook, viz., 156° 00′. The true longitude of Cook's observatory there is about 155° 55′ 30″. Vancouver gives for the longitude of his anchorage off Waikiki, Oahu, 157° 50′ 23″ w., which is only half a mile too far west, and for Wiamea, Kauai, 159° 40′ w., which is probably within a quarter of a mile of the true longitude.

Captain Freycinet, of the scientific exploring ship *L'Uranie,* in 1819, made the longitude of Kealakekua 156° 04′ 23″.5 w. from Greenwich, which is 36 s. too far west. He made the longitude of Honolulu 157° 51′ 46″.2 w. from Greenwich, which is about 1 s. too far east.

Commodore Chas. Wilkes, of the U. S. exploring expedition, in 1840, adopted Captain Cook's longitude of Kealakakua Bay, viz., 156° 00′ w., and placed Honolulu at 157° 54′ w., and Waimea, Kauai, at 159° 44′ 30″, both of them 8 s. too far west.

During the years 1845–6 the late Professor Chester S. Lyman, afterwards a professor at Yale University, who was then residing in Honolulu for his health, assisted Mr. David Flitner, chronometer maker, in establishing a small observatory here, and made a series of meridional observations of the moon, in order to determine the longitude. The result he obtained, using the *predicted* places of the moon in the American 'Ephemeris,' was 10 h. 37 m. 15 s. w., or 12 s. too far east.

In the year 1868, M. Fleuriais, in the service of the 'Bureau des Longitudes,' who came to observe a transit of Mercury at Honolulu, established an observatory near the Catholic Cathedral, where he observed nineteen meridional transits of the moon's first limb, and eight of the second limb. These observations are published in detail in the appendix of the *Connaissance des Temps* for 1872. The result, as first published, was 160° 10′ 38″ west of Paris, or 157° 50′ 23″.5 w. of Greenwich, or 5.4 s. too far east.

But in No. 2586–7 of the *Astronomische Nachrichten,* for April, 1884, we find a reduction of Fleuriais' observations for longitude, carried around the world in 1867–70. On pages 345–6 are given the single results obtained in Honolulu in October–December, 1868, 27 in number, and the longitude deduced from them is 10 h. 31 m. 25.59 s., or 1.3 s. too far east. These results were obtained by combining the observations at Honolulu with the actual observations of the moon's place made during the same period at Washington, Greenwich and Paris.

In September, 1874, Captain G. L. Tupman, Royal Marine Artillery, in charge of the British Transit of Venus Expedition of that year, arrived in Honolulu, and established an observatory on Punchbowl Street, near the shore, on practically the same meridian as C. S. Lyman's observatory, and 4″.79 west of Fleuriais' pier. No pains were spared to ascertain the longi-

tude by lunar observations, and the accuracy of the work has since been fully confirmed.

The observations for longitude were continued through the months of October, November and December, 1874. During this time over 700 meridional transits of the moon, thirteen occultations of stars, and about 540 zenith distances of the moon's upper and lower limbs, combined with those of well-known stars near the moon, were observed. The first reduction of the observations apparently agreed with the result obtained by Professor Lyman, viz., 10 h. 31 m. 15 s., but after returning to Europe and correcting the tabular right ascension by the contemporary observations made at Washington, Greenwich, Paris, Königsburg, Strasburg and the Cape of Good Hope, Captain Tupman increased the result by about twelve seconds of time. His final result by meridional transits of the moon was 10 h. 31 m. 26.0 s.; by zenith distances, 10 h. 31 m. 27.3 s.; by occultations of stars, 10 h. 31 m. 26.9 s.

The result officially communicated to the Hawaiian Government Survey was 10 h. 31 m. 27.2 s., upon which all the maps since then have been based. It is interesting to note that this value agrees to the tenth of a second with the latest determination of the longitude.

Captain Tupman, however, afterwards weighted the above results according to the number of observers employed on each, giving the occultation result the weight 5, the mean of the first two results the weight 4 and M. Fleuriais' result the weight 1, on which conditions the resulting longitude is 10 h. 31 m. 26.3 s. If he had used the value deduced from Fleuriais' work by the *Astronomische Nachrichten*, his final mean would have been 10 h. 31 m. 26.7 s.

In March, 1875, Captain Tupman made an attempt to connect Honolulu with San Francisco by transportation of chronometers. Accordingly twenty chronometers were carried by H. B. M. S. *Reindeer*, Commander C. V. Anson, from Honolulu to the U. S. Navy Yard, Mare Island, and compared with the local time at both stations. Unfortunately the *Reindeer* was blown out of her course by a northerly gale, which lengthened her voyage seven or eight days, and lowered the temperature in the chronometer boxes as much as 15° F. Hence the resulting determination of the longitude of Honolulu, viz., 10 h. 31 m. 33.2 s. ± 3.0 s. w., had very little value.

Again, in August and September, 1884, an attempt was made by the Hawaiian Government Survey with the cooperation of Professor Davidson and Mr. Morse in San Francisco, to determine the longitude of Honolulu by comparing the chronometers on board the O. S. S. Co. steamer *Mariposa*, with the local time at each end. This was done for two round trips, giving a mean result of 10 h. 31 m. 25.8 s.

In view of all the above facts, the Hawaiian government adopted 10 h. 31 m. 26.0 s. as the most probable value, in rating chronometers and furnishing standard time, but not for mapping, until the recent telegraphic determination made by the U. S. Coast and Geodetic Survey.

JOHN F. HAYFORD.

SCIENTIFIC BOOKS.

A Discussion of Variable Stars in the Cluster ω Centauri. By SOLON I. BAILEY. Annals of the Astronomical Observatory of Harvard College, Vol. 38. 4°. Cambridge, Mass. 1902. Pp. 252; 13 plates.

Among the most interesting discoveries in the subject of variable stars during the last decade belongs the finding of an exceptionally large number of variables in certain globular star clusters. The remarkable fact that in many of these systems a not insignificant proportion of all the stars change their light

in a quite regular way, and that among the objects of one and the same system the elements of the light changes, especially the length of period, amplitude of variation and form of light-curve, show certain common characteristics, impels one to the acceptance of a common, or at least similar, cause for the changes of light. To be sure, up to the present time, no entirely satisfactory explanation has been found; but precisely this enigmatical character of the phenomenon, which, without doubt, stands in a certain relation to the stage of development of the cluster concerned, increases the interest in itself, and incites to further research on this new subject.

The credit for the discovery of the many variables in the star clusters belongs to Professor Bailey, who is in charge of the Arequipa Station of the Harvard Astronomical Observatory. This credit is much enhanced from the fact that the discoverer has taken upon himself the enormous task of studying the light changes of this increasing number of variables, and of determining their elements. The first fruit of this undertaking is the present volume, which is especially concerned with the cluster *ω Centauri*. The unwearied industry and great skill which the author, and his colaborer, Miss Leland, have shown in the measurement and study of the rich materials of observation, will call forth special acknowledgment and admiration, and one may well congratulate the Harvard Observatory on a publication which commands a prominent place among the valuable works of this institution.

This work bears eloquent witness also to the high importance which the application of photography has won in the development of astronomy at the present time. In general only by photographic means was the solution of the above problems possible. It is scarcely conceivable that in the densely crowded star clusters any such valuable results could have been obtained by direct estimates of brightness or measures in the telescope. Even the identification of the individual objects would present the greatest difficulties, and the precision of the observations would be injured by the troublesome nearness of the other stars, quite aside from the great expenditure of time which the execution of the work would require. On the other hand, a single photographic plate renders possible the determination of the brightness of all the variables in the cluster. By means of réseaux, by the use of enlargements, and the selection of special portions of the photographs, the discovery of the various variables is notably simplified. The danger of confusion can thus be wholly eliminated, and the first comparison of brightness can be checked by another determination, and improved at will by the independent estimates of another observer.

In the discussion of this comprehensive volume one naturally can not enter into all the details; and in what follows only the most important points in regard to the reduction of the observations and the most weighty conclusions are discussed somewhat carefully.

In the first chapter the author gives, first of all, a brief historical sketch in regard to the discovery of variables in the different clusters. The surprising phenomenon was first observed in the two clusters Messier 3 and Messier 5. In the first, Pickering, in the year 1889, had discovered a star near the center of the cluster, which he at first regarded as a nova, but which later proved to be an ordinary variable. In the second, Packer, in 1890, found by direct observations the variability of two stars, which was verified at the Harvard Observatory by means of photographic plates. Also in the same year Common made the discovery of several variable stars in this cluster. An examination by Mrs. Fleming and Professor Pickering of photographs made at Arequipa led in August, 1893, to the discovery of two variables in *ω Centauri*, and soon after followed the finding of six variables in 47 *Tucanæ* by Professor Bailey and Mrs. Fleming. In the year 1895, after Pickering and Bailey had succeeded in establishing the variability of 26 stars in *ω Centauri*, a systematic investigation of the densest star clusters was undertaken at the Arequipa Observatory, and this search brought to light a surprisingly large number of variables. A summary of all the objects discovered up to 1898 was given by Pickering in the

Harvard circular No. 33. The same is again published on page 2 of the present volume. In the 23 clusters which had been examined, 19,050 stars were compared, and not less than 509 variables were found. The distribution among the different clusters is very different. In two of them no variable was found, in three, only one; in four, only two; and so on. The two clusters Messier 3 and *ω Centauri* show the greatest number of variables. The first has 132 among 900 stars examined, the second, 128 among 3,000 stars compared; in the first case there is one variable in every seven stars, and in the second case one in every twenty-three.

In the present volume only the cluster *ω Centauri* is discussed, for which more material was available than for the others. In regard to number of stars it is the most striking cluster in the sky, and, on account of the brightness of the individual stars, photographs of it can be obtained with shorter exposures than of the other clusters.

To the naked eye it appears as a hazy star of the fourth magnitude. Its form is elliptical. A count of the number of stars, made in the year 1893, gave for the best photographs, 6,389. The diameter was taken as 35′, but a few variables were found somewhat more distant from the center, and as it appeared probable that these really belonged to the system, the borders of the clusters were thus somewhat extended, even to a diameter of 40′. In regard to the magnitudes of the stars, there is no star brighter than 8 magn., less than 100 stars between 8 magn. and 12 magn., and more than 6,000 between 12 magn. and 14½ magn. A large number of the stars which were counted may perhaps not belong to the system, but are only accidentally projected upon it.

The photographic plates which furnished the basis for the investigation were nearly all made with the thirteen-inch Boyden refractor, which for the photographic rays has a focal length of 191.5 inches. On the original plates, therefore, 1 mm. is equivalent to 42.4″. The images of the fainter stars have a mean diameter of 2″. This quantity, however, varies considerably on different plates. For keeping the position of the stars fixed on the plates no finder was used, but an eye-piece, which was inserted near the plate, in the field of the main telescope itself. A few plates were made with the Bruce 24-inch telescope, and with the 11-inch Draper telescope, which are of less focal length than the 13″. The time of the exposure which was necessary at Arequipa, in order to show the faintest variables when at minimum, was for *ω Centauri* 30 minutes. For the fainter clusters notably longer exposures are necessary: in the case of Messier 5 about 50 minutes, for Messier 3 about 100 minutes, and for others even two hours. In most clusters the central portions are so densely crowded with stars that a detailed study is quite impossible.

The method which was pursued by Bailey in the discovery of variables in clusters was substantially as follows: The particular cluster was divided into a number of parts each of which contained about ten stars. These stars were arranged in a so-called sequence according to their brightness, from the brightest to the faintest. This sequence was then memorized and as large a number as possible of original plates (if possible, at least ten) was tested and compared as to how and to what extent the particular sequence was changed. In this way the whole cluster was examined piece by piece. This labor is extremely tiresome. It demands a rigorous examination of numberless plates under a microscope of considerable power, in order to separate the thickly crowded portions, and extraordinary care not to confuse the different stars with one another. In all cases where the variation is small, or where the period is about twenty-four hours or a fraction thereof, the detection of the variability was difficult. On this account many variables may have remained undiscovered. Especially difficult is found the examination of the central portion, where the star images coalesce, and precise comparisons are impossible. On this account it happens that, especially in the densest clusters, relatively few variables are found near the center. The discovery is also rendered difficult from the fact that the duration of the maximum phase is only a com-

paratively short part of the whole period (sometimes only one fifth). For, since in the remaining time no noticeable changes occur, the variability is made apparent, in many cases, only by the examination of very many plates.

In order to determine the brightness of the variables in the cluster *ω Centauri,* in the first place, a system of 38 comparison stars was selected, whose brightness ranged between magnitude 9.0 and 14.6, and which lie either within the cluster itself or at least in its immediate neighborhood, in any case not farther from the center than the most distant variables. The mean difference in brightness between two successive sequence stars is about 0.15; for the brighter stars the interval is in general larger, while the fainter stars lie closer together. The method which was employed in the determination of the magnitudes of the comparison stars is that which Pickering has described in Volume 26, Part II. A comparison scale was first made in which by the use of a selected star a line of images was obtained on the same plate by clockwork, and also with exposures of different lengths. The star was first exposed for 810 seconds, then the telescope was moved by a slight amount in right ascension and the star again exposed, this time for 270 seconds. Four other images with exposures of 90, 30, 10, and 3 seconds serve to complete the scale. On the supposition that the photographic effect is proportional to the time of exposure, any two successive images of the six star impression on the plate would differ by about 1.2 magnitudes. That portion of the plate which contained the six images was cut out and, protected by a glass cover, was fastened in a small frame, and could be placed on any other plate. By the help of such a comparison scale the determination of the brightness of the 38 selected comparison stars was made by placing first the brightest of them between the two images of the scale, which seemed to stand next to it in intensity, by which means the difference of the interval was estimated to tenths. The same process was then performed for the second star, and in this way the difference in brightness between the two comparisons was determined in magnitudes. In just the same way the second comparison star was joined to the third, the third to the fourth, and so on, each one with the one following it in brightness. The whole work was not, moreover, carried out by the use of a single scale, but in all four different scales were used, and, moreover, each pair of stars was measured on several plates, for the most part on four. All the measures were independently made by Mrs. Fleming as well as by Miss Leland. In order to arrange all the brightnesses in magnitude, values were taken for the first three, which accorded with the photometric system of the Harvard Observatory, and the magnitudes of the remaining 35 stars were then obtained by the use of the differences found for the single pairs of stars, after the application of an unimportant correction, which was made on the assumption that the first three stars belonged to a different spectrum type from the others. The final magnitudes of the 38 comparison stars are given on page nine, in the last column of Table III.

It is obvious without extended discussion that the whole method of the determination of the brightness of the comparison stars does not permit the attainment of the highest degree of accuracy. Aside from the fact that the ratio between the times of exposure and the brightness of the images is not rigidly exact, as a result of which the different images do not differ from each other by precisely 1.2 magnitudes, the comparisons between two images of the scale is a somewhat uncertain operation. It is, therefore, not to be wondered at that the various columns of determinations show in part very strong systematic differences from one another, not only between the two observers, but also with the same observer on different plates, and especially with the use of different scales. Apparently the appearance of the star images played an important rôle. Also the different scales, on account of dissimilar atmospheric conditions, may give a noticeably different result. Perhaps it would have been better for the precision of the comparisons to have made the steps between successive images of

the scale less than 1.2 magnitudes, and yet more desirable would it have been for the formation of the scale to have made use, not of change in the time of exposure, but rather of the cutting down of the objective (which Pickering has already made use of on another occasion), not, however, through the use of circular caps, but by sectors, or to have made use of polarization methods for decreasing the light for the formation of the scale. It is thus only indicated by what method it may be possible to obtain greater accuracy. The Pickering method for the determination of photographic stellar magnitudes in and for itself in the present instance the reviewer regards as thoroughly commendable. It is without doubt preferable to various methods, especially to that of measuring the diameters of the stellar images.

In the second and third chapters are given the measurements of brightness of the variables in the cluster *ω Centauri*. The photographic plates employed for this purpose cover the time from May 15, 1892, to August 16, 1898. In all 128 variables were measured. At first 132 objects were selected as variable, and were designated with the numbers 1–132. But later the four stars Nos. 28, 31, 37 and 93, which appeared not sufficiently sure, were rejected. The extremely wearisome comparisons, which made the greatest demands on the endurance and skill of the observer, were made for the most part by Miss Leland. A smaller number of plates were measured by the author himself, especially for the purpose of determining the provisional periods. The measurements were made in such a manner that the variable was estimated between two comparison stars, one of which was brighter, the other fainter than the variable. The difference in brightness was estimated in grades. The method is thus the same as the well-known Argelander method of comparison by grades, which has proved so effective in direct observations of brightness in the sky. The value of a grade was not far from 0.1 magnitude. Generally only two comparisons were made. Only when the variable appeared equal to one of the comparison stars, two other comparisons were made, one with the fainter and the other with the brighter comparison star. All the observations for each of the 128 variables are given in tabular form on pages 17–124 of the volume. In all there are 15,000 determinations of brightness, and since on the average each determination consists of two comparisons, there are about 30,000 comparisons for bringing the results into form. In the tables are give the individual comparisons in grades, and also the derived magnitudes of the variables, as well as the residuals of the single estimates from the mean, and also the phase, that is, the time between the mean time of the exposure and the preceding computed maximum, and finally the residuals of the observed magnitudes from the mean light curve found for the star in question. When this residual was 0.40 or more the measurement was repeated, and in case the new measurement brought no sure conclusion it was checked still a second or third time, eventually also by another observer.

Most interesting is the fourth chapter, which contains the results of all the observations. In the tabulated grouping of the elements of the light changes are found only 95 examples out of the 128 variables. For 13 variables no periods at all were found, and for 20 objects the results obtained were so doubtful that they are not given in the table itself, but are mentioned only in the remarks which follow the table. In regard to the method of determining the periods, the epochs, the extremes of brightness and the mean curves, no description at all is given in this chapter, and one only finds in the whole volume some brief notes on page 232. The method of computation is, however, so well known in such cases that a detailed description would perhaps be superfluous. The plates made on the successive days, May 3 and May 4, 1898, eight plates on the first, and six on the second date, which cover periods of seven and five hours, respectively, give for nearly all the variables a sufficient indication of the character of the light changes, and render possible in most cases the finding of an approximate value of the period. An improvement on this approximate period can generally be obtained

without great difficulty by the use of several groups of plates which in greater number were made on closely associated dates, as, for example, those of 1895, from May 31 to June 30, and of 1897, May 13 to June 17. So by degrees the period can be continually improved, until eventually a use of the widely separated observations is possible, with the use provisionally of a derived light curve, which then gives the final sure value of the period. When this value is once found, and an initial epoch determined, one can at length for each individual measure find the difference in time from the preceding maximum or minimum, arrange the magnitudes according to these differences, unite the several adjacent ones to means and by the help of these normal magnitudes construct graphically the mean light curve.

The light curves obtained by the author for the reasonably sure variables are reproduced in Plates II. to VII. at the end of the volume, and give a glimpse at the peculiarities of the light changes for the individual objects. The scale of these curves is chosen somewhat small. By a somewhat larger representation clearness would have been gained. The residuals of the individual observations from these curves have already been given in the table of observations in Chapter III. Also in Chapter IV., in a special table approximate values are found for the duration of the maximum and the minimum phases, as well as for the increase and the decrease of the light, and for the rate of increase and decrease in magnitudes per hour, and finally the mean residual, that is, the mean of the residuals from the curves without regard to sign. The last value varies between 0.07 and 0.25 magnitude, the mean being 0.128.

In order to obtain an idea as to the certainty with which the elements of the light changes may be derived from the available materials, the writer selected at random several stars, and without knowing beforehand the results obtained by the author, independently determined the elements. The results agreed closely in all cases with those in the present volume. One may, therefore, put full confidence in them. One who has himself carried out such computations and knows thereby how many attempts and approximations must be made before a satisfactory arrangement of all the materials of observation is reached, will at best be able to appreciate what enormous labor is involved in his table of elements.

A glance over this table shows that for nearly all the stars the length of period is only a fractional part of a day. Only 5 stars make an exception, which have periods of 1.35, 14.75, 29.34, 297 and 484 days, respectively. For 26 stars the periods are less than 10 hours; No. 19 has the shortest period, 7.2 hours.

The range of light variation is noticeably different for the different variables. The difference between the maximum and minimum brightness varies between 0.45 and 4.58 magnitudes, and in general also, as with variables not situated in clusters, the longer the period the greater is the amplitude. A range of less than 1 magnitude for the determined difference is found in the case of 56 stars. Where the amplitude is less than 0.6, or perhaps 0.5 magnitude, the determination of the light curve is naturally somewhat uncertain, and one must look upon these objects with some doubt. It ought not to be overlooked, however, that frequently the real changes in brightness will be somewhat greater than those given in the table, since for many stars the brightness changes with great rapidity at maximum, and on this account the photographs, especially when the exposure is very long, do not record the true maximum brightness, but one something less. Especially is the relatively long duration of the exposure, which for these swiftly changing objects may be a considerable part of the whole period, a provoking hindrance to the determination of the genuine light phenomena.

Of great interest is the study of the light curves. These are by no means alike for all the variables in the cluster. They are, however, always of certain types. The author has distinguished three different classes of light curves. The first class, in which 37 stars may be reckoned, is characterized by the fact that

the star remains at minimum during half of the period, perhaps with very slight fluctuations. Moreover, the light curve is uniform, the increase of light very rapid and the decrease noticeably slower. The amplitude of the light variations attains in general 1 magnitude and a little more, and the periods lie between 12 and 15 hours. The second class embraces the relatively small number of 19 stars. They differ from the first only in the fact that the long duration of minimum is absent, and that in general the increase is much slower. The decrease continues with ever-lessening rapidity till the beginning of the following increase. In many cases on the descending arm of the curve a 'stillstand' appears to exist. The whole range in brightness is generally less than 1 magnitude, and the periods range between fifteen and twenty hours. In the third class are counted about 34 stars. The peculiarities of these stars are that the durations of increase and decrease are not very different; also, the increase is in general somewhat shorter than the decrease, but in some cases they are equal, and it even happens that the opposite is true. The amplitude of the variation is for the most part not much greater than 0.5 magnitude, and the periods are between eight and ten hours.

As one sees, the three classes are not separated very widely from one another; especially, the second is only a little different from the first, and there are transitions from one to the other, but as a whole the differences of the changes in light, especially between the extreme members of the classes *a* and *c*, are marked. The circumstances by which the variations of light take place among the different members of the cluster, whether due to occultation, rotation or to other causes, must in any case be very different.

In regard to many peculiarities of the light curves of the different stars the remarks at the end of Chapter IV. give information. Notes are also found in regard to the doubtful objects for which no elements are given in the table.

The fifth chapter is devoted to a special study of four variables of the cluster. The author aimed to prove by this special investigation whether noticeably better results could be obtained if the number of comparisons of brightness was increased, and especially if comparison stars, greater in number, and in the immediate vicinity of the variable, were employed. For each of the selected variables a list of comparison stars was chosen, no one of which was more than 2′ distant from it. The variable was then compared with as many of these as possible, and the difference in brightness determined each time in grades. All the new measures were made independently by the author and by Miss Leland. The magnitudes of the new comparison stars were determined on four plates by joining them to the previously chosen fundamental stars, and also in this case by both observers independently.

In addition to these direct determinations, the brightness of the new comparison stars was also found by an indirect method, by which, as is done in visual comparisons of variable stars, by the comparison of the variables with the comparison stars the differences in brightness of these stars in grades were derived; and then by the union of the various differences a scale of grades was formed. The union of the values of the comparison stars, obtained in these two different ways, furnished at length definite values for them, from which the results of both observers were united to form mean values. The differences between the two observers are in general small, although there are indications of systematic differences. The magnitudes of the four variables were derived from the observations simply by the Argelander method, by the help of the scale of grades, first in grades and then in magnitudes.

The result of this somewhat complicated and prolix research was, in the case of three of the selected variables, a complete accordance with the previously derived elements, in which, naturally, as might be expected from the large number of the measures and especially from the greater nearness of the comparison stars, the residuals of the single observations from the light curve resulted somewhat smaller. The case was different with the fourth star, which of all the variables

in the cluster has the smallest range of brightness. For this variable, at first, a proof of 0.275 days had been derived, which, indeed, satisfied a large part of the observations. The new investigation gave a distinctly different value, 0.37987, and, moreover, made it probable that the period is not constant, but, within the time which the observations cover, varies between 0.3796 and 0.3801 day. At least with this assumption a noticeably better representation is obtained. Since in the case of this star the changes of brightness are not much greater than the unavoidable errors of measurement, it is not very surprising that the new determination, made with other comparison stars, has led to an entirely different result. The limit is here reached where the precision of the method employed in the determination of the brightness fails, and it appears more than doubtful whether the new result deserves more confidence than the old. It would, indeed, be wiser, where the light fluctuations are so small, not to attempt the determination of the period.

Under the heading, 'Miscellaneous Results,' in the last chapter is discussed a number of questions which are concerned with the precision of the measurements, the distribution of the variables in the cluster, the light curves, etc. Some of these investigations deserve to be taken up briefly.

The distance of the stars from the center of the cluster has a marked effect on the precision of the comparisons. The nearer the variables are to the center, and the more, on this account, the images run together on the photograph, so much more difficult also are the comparisons of brightness. Within a distance of 5′ from the center of the cluster are found 46 variables; for 16 of these, on account of the difficulty of the observations, no period at all could be found, and for the remaining 30 the mean deviation from the corresponding curves is for the whole ± 0.159 magnitude. On the other hand, for 35 variables whose distance from the center lies between 5′ and 10′, the corresponding mean deviation is only ± 0.114 magnitude, and for 30 stars, whose distance is greater than 10′, it is only ± 0.113 magnitude.

When the deviations of the individual observations from the mean light curve attained a value of 0.40 magnitude and greater, the observations were repeated. In certain cases it happened that errors of measurement existed, which were due to errors of identification, probably. Very many large residuals are found, however, in all 287 cases, which are verified by the measurements of the revision. These residuals have their cause, perhaps, in real irregularities of the light curve, perhaps also in defects on the photographic plates. The general quality of the plates has naturally an influence on the precision of the results, and in general the sharpest and best plates give the smallest residuals; while with very experienced observers and with the extremest care the gain is not so important as one might expect beforehand.

Since it is known that in case of comparisons of brightness of two stars in the sky their relative position plays an important rôle, it was of importance to investigate whether in the determinations on photographs the relative position of the two star images might exercise a marked effect. To find the answer to this question one of the variables was compared on several plates with a number of comparison stars, while, by turning the plate under the microscope, the comparison star was placed first on the right and then on the left of the variable, and each time the difference in brightness was estimated in grades. It was thus shown that the influence of the change in position, if it exists at all, is so small that it can be completely ignored.

Of special interest is the investigation concerning the distribution of the variables in the cluster. The author has for this purpose divided the variables into twelve groups, of which the first includes all to a distance of 2′ from the center, the second those whose distance lies between 2′ and 4′, etc. For each group the number of variables per square minute was computed. The following table contains the results for the different groups. Next to them are given the corresponding numbers for the distribution of all the stars of the cluster, which were taken from a previous publication in the *Harvard Annals*. The

numbers in the last column, which give how many variables are found in the different groups for each 100 stars, are not noticeably different from one another. It follows, therefore, that in the whole cluster, in a definite area the proportion of variables is about constant, and that in no place occurs a very large number.

Variables.			All Stars.		
Mean Distance of Group.	Number.	Number per Square Minute.	Mean Distance of Group.	Number per Square Minute.	Per Cent.
1′	7	0.556	1.1′	42.8	1.3
3	26	0.690	3.1	32.3	2.1
5	21	0.334	5.2	19.7	1.7
7	17	0.193	7.1	12.3	1.6
9	17	0.150	9.1	6.5	2.3
11	16	0.116	11.1	3.9	3.0
13	9	0.055	13.3	2.8	2.0
15	6	0.032	15.0	2.1	1.5
17	4	0.019	16.7	1.6	1.2
19	4	0.017	18.4	1.1	1.5
21	0	0.000	20.2	0.7	—
23	1	0.003	—	—	—

In regard to the three classes into which the variables were divided, the mean distances of the stars from the center of the cluster of the different groups are 7.7′, 8.1′ and 8.9′. It appears, therefore, that no marked difference is indicated in the distribution, on account of the character of the light changes. The mean maximum brightnesses of the three classes are 12.99, 13.10 and 13.33 magnitudes, and the minimum, 14.11, 13.97 and 13.89 magnitudes. The amplitudes are thus distinctly different. The mean lengths of the periods are 0.58617, 0.75153 and 0.39463.

It is of great value that for the 95 variables whose periods are determined the light curves are given not only graphically in Plates II. to VII., but that the coordinates of the light-curves are given in detail on pages 210 to 222, and in such a way that for each star the period is divided into 24 equal parts, and for each part the corresponding brightness is given. There are also given typical light tables for the three classes, by taking from each class five especially characteristic examples, and taking the mean values for these.

The chapter closes with some remarks concerning the possible causes of the light changes of so many stars in one and the same cluster. The author hesitates to accept widely current hypotheses, and to test them from the available materials of observation. He concludes only that from the complete uniformity of the periods, which is shown in *ω Centauri* for a long time, the variability must be associated with some regularly returning phenomenon, either through regular eclipses by bodies which are in revolution about each other in definite paths, or through the rotation of unequally illuminated or irregularly shaped bodies. The explanation by means of occultations, as with Algol stars, involves great difficulties. For, if one would assume, as at first sight might appear plausible, that the planes of the orbits of the different double star systems of the cluster lie about parallel to each other, and that on that account the light-curves of the different systems should show a certain similarity, the form of the light-curves, nevertheless, speaks against the eclipse theory. In *ω Centauri* are found three different types of light-curves, but in other clusters investigated by the author the first type with the long duration of minimum is so much more common that we may regard this as the characteristic type of variables in star clusters. This type of light-curve is, however, entirely distinct from the Algol type. The eclipse would have to last for a considerable part of the period, and this would hardly be consistent with any orbit system. Also the assumption of an axial rotation with unequally luminous surfaces seems not very probable, when one considers that such a large number of similar variables is concerned. The whole phenomenon is at this time somewhat enigmatical, and we must await the investigation of the occurrences in other clusters before further conclusions are permissible.

In an appendix to the volume are given preparatory studies toward the measurements of all those clusters in which more than one variable has been found. There are given the positions of those objects selected to serve as fundamental stars, and the comparison stars

chosen for the comparisons of brightness, and the stars already known to be variable, all the positions being given in seconds of arc, and determined from the center of the particular cluster. In Plates VIII. to XII. are given reproductions from the original plates of fourteen clusters, on which the variables and comparison stars are marked. On these reproductions 1 mm. equals about 10″. Especially interesting are the repeated enlargements of certain portions of some clusters, which are given in the last plate of the volume. These show clearly the change in appearance of the variables on different plates, and give an idea of the certainty with which the comparison with adjacent stars can be made.

From the materials given in the appendix one sees that there still remains a very great amount of labor to be done. We hope that the author will be able to carry out his plan, and to give as clear and exhaustive a discussion of the light changes in the other star clusters, as he has done in the present volume.*

G. Müller.

SCIENTIFIC JOURNALS AND ARTICLES.

The contents of the *American Journal of Science* for November are as follows: 'Mineralogical Notes,' by C. H. Warren; 'Studies of Eocene Mammalia in the Marsh Collection, Peabody Museum' (with plates XVI. and XVII.), by J. L. Wortman; '*Tridenum Virginicum* (L.) Rafin,' a morphological and anatomical study (with figures in the text), by T. Holm; 'Ephemeral Lakes in Arid Regions,' by C. R. Keyes; 'Note on the Identity of Palacheite and Botryogen,' by A. S. Eakle: 'Colloidal Gold: Absorption Phenomena and Allotropy,' by J. C. Blake.

SOCIETIES AND ACADEMIES.

AMERICAN CHEMICAL SOCIETY. NEW YORK SECTION.

The first meeting of the season was held at the Chemists' Club, No. 108 West 55th Street, on Friday evening, October 9.

After a few remarks by the chairman, Professor Miller, outlining the policy of the section for the ensuing year, and requesting members to present papers in abstract as far as possible, so as to have more time for discussion, the following papers were read:

The Volumetric Determination of Zinc: W. J. Waring.

This paper was read by Mr. Stone and discussed by Messrs. Brenneman, Stone, Miller and Danziger. It called attention to the widely differing results which are obtained by different chemists in the determination of zinc by the ferrocyanide titration method, and pointed out the necessity of uniformity in the conditions of standardizing and titrating, so that the composition of the precipitate shall be uniform. The occurrence of cadmium in the ores of the Joplin District in amounts varying from 0.1 to 2 per cent. was shown to interfere with the accuracy of the method, so that the cadmium should be removed, best by aluminum foil, before the titration. A new cadmium ammonium ferrocyanide was also described.

The Reduction of Lead from Litharge in Preliminary Assays and the Advantages of an Oxide Slag: E. H. Miller, E. J. Hall and M. J. Falk.

Professor Miller gave an abstract of an article which will soon appear in the *Transactions* of the American Institute of Mining Engineers. It was shown in making preliminary assays to determine the reducing power of an ore that, not only did the amount of lead reduced vary with the acid or basic character of the slag, but that the amount of lead oxidized by niter varied with the reducing agent present, even under uniform conditions as to charge, time and temperature. This was not anticipated, and explains the difficulty in the old preliminary assays.

The best results were obtained by using a charge of ore 3 grams, litharge 50 grams, soda 10 grams, no silica, no borax glass and no salt cover. With this charge and a temperature of over 900° C. the sulphur is completely oxidized to sulphate and forms an upper layer in the slag (Na_2CO_3 and Na_2SO_4), while the lower layer consists of a readily fusible mixture of oxide of lead, of iron, etc.

* Translated from *Vierteljahrsschrift der Astron. Gesellschaft*, 38. Jahrgang, Erstes Heft, 1903.

This style of charge was then tested with a quantity of ores containing sulphur combined with iron, copper, zinc and lead. The charge for the final assay was ore 1/2 assay ton, litharge 70 grams, soda 15 grams. Niter as calculated for a 20-gram button (from the results of the preliminary assay). The buttons were soft, malleable and weighed from 17–23 grams, while the results in gold and silver were slightly higher than the old methods and the loss in the slag slightly less.

The Influence of Diet, Muscular Exertion and Loss of Sleep upon the Formation of Uric Acid: H. C. SHERMAN.

Observations made in connection with metabolism experiments upon three professional athletes and one subject of sedentary habits showed the quantity of uric acid eliminated to be primarily dependent upon the quantity of meat products in the diet, and to be influenced very little, if any, by the abundance of a bread and milk diet, by a considerable loss of sleep, or (in the case of the professional athletes) by long-continued muscular exertion. With the subject of sedentary habits, a much smaller amount of exercise increased slightly the uric acid elimination. This paper will appear in the November issue of the *Journal of the American Chemical Society.*

H. C. SHERMAN,
Secretary.

ELISHA MITCHELL SCIENTIFIC SOCIETY.

AT the 150th meeting of the Elisha Mitchell Scientific Society, held in the Chemical Lecture Room of Person Hall, University of North Carolina, October 13, the following papers were presented:

The Use of the Vector Diagram in Electrical Engineering: Mr. J. E. LATTA.

Tanning (with specimens): Professor CHARLES BASKERVILLE.

After outlining modern methods of tanning, especially by the use of chromium nitrite, a number of rare skins which had been done for Messrs. Tiffany & Co., of New York, were exhibited. The skins were presented to the Museum of the Chemical Laboratory.

The Influence of the Spermatozoon on the Larval Development of the Sea-Urchin: Professor H. V. WILSON.

A New Indicator: Professors E. V. HOWELL and A. S. WHEELER.

A new indicator extracted from the hulls of the muscadine or wild Bullace grape was announced. This coloring matter gives a red color with acids and green with alkalies, being purple in neutral solutions. The only solvents so far found which may be used for its extraction are alcohol and water. It responds to inorganic and organic acids and volatile and non-volatile alkalies. Carbon dioxide does not affect it.

On adjournment of the public session, the annual meeting was held for the election of officers and transaction of business. The proposed agreement with the North Carolina Academy of Science was approved. By this agreement the *Journal of the Mitchell Society* becomes the official organ of the North Carolina Academy of Science, its size being doubled and issued quarterly. The following officers were elected for the ensuing year:

President—Professor Charles Baskerville.
Vice-President—Mr. J. E. Latta.
Recording Secretary—Professor A. S. Wheeler.

President Venable retains the permanent secretaryship.

CHARLES BASKERVILLE,
Secretary.

DISCUSSION AND CORRESPONDENCE.

A HITHERTO UNDESCRIBED VISUAL PHENOMENON.

TO THE EDITOR OF SCIENCE: The phenomenon of apparent movement described by Dr. Gould (SCIENCE, XVIII., 536) was discussed in 1896 by Professor S. Exner in an article entitled 'Ueber autokinetische Empfindungen' (*Zeits. f. Psych. u. Physiol. d. Sinnesorgane,* XII., 313). According to Exner, the first observation on record was made by Alexander von Humboldt in 1799. Several authors (among them men as well known as Aubert and Charpentier) have occupied themselves with the phenomenon; and it forms the subject of an experiment in Sanford's *Laboratory Course,* 1898, 309.

E. B. TITCHENER.

SHORTER ARTICLES.

PHOTOTROPISM UNDER LIGHT-RAYS OF DIFFERENT WAVE-LENGTHS.

THE effect of lateral incidence of light upon *Cormophytes* is of such a nature as to produce a tendency in the plant to arrange its axis parallel to the direction of the incident ray. This response to the stimulus of light is quite general with regard to higher plants, and has long been known under the name *heliotropism*. The term *phototropism*—from its literal meaning more appropriate—has recently been introduced to displace the older term.

The relative phototropic effects of rays of different wave-length have been given by Wiesner* to be greatest between ultra-violet and violet rays, diminishing gradually over to the yellow, where it disappears, then beginning in the orange and reaching a small secondary maximum in the ultra-red. Guilleman's† results resembled those of Wiesner excepting with respect to the yellow. He concluded that curvature takes place under all the rays except the least refrangible heat rays. Sachs himself states that under blue light curvature takes place as in ordinary daylight, and that no curvature whatever takes place behind a ruby-red glass; and he agrees with Wiesner that no curvature takes place behind a yellow screen.

With regard to the decoloring effect upon a fresh alcoholic solution of chlorophyll by rays of different wave-length, the present position is practically that expressed by Vines:‡ 'Sachs and Wiesner have ascertained that the rays of low refrangibility are more active in forwarding it than those of high refrangibility.'

Some investigation of these subjects has been made by the writer, and the results are here given because they differ materially from those referred to, and because it is thought the methods here used to test the matter are less open to objection and more complete than those of the authors mentioned above.

The following named glass plates (each nine by twelve inches) were secured from Bausch and Lomb and are what are commonly called standard 'colors'—*violet, blue, green, yellow, red*—several of each. Window-glass was used to admit daylight, and sheet iron for the opaque screen. These colored plates were examined by the writer with a view to getting the particular spectrum of each, because colored glass can scarcely be represented accurately by simply *naming* the 'color'; for certain colored screens allow other 'colors' to pass than that which would seem from ordinary observation. The curve for each 'color' is plotted approximately in the accompanying

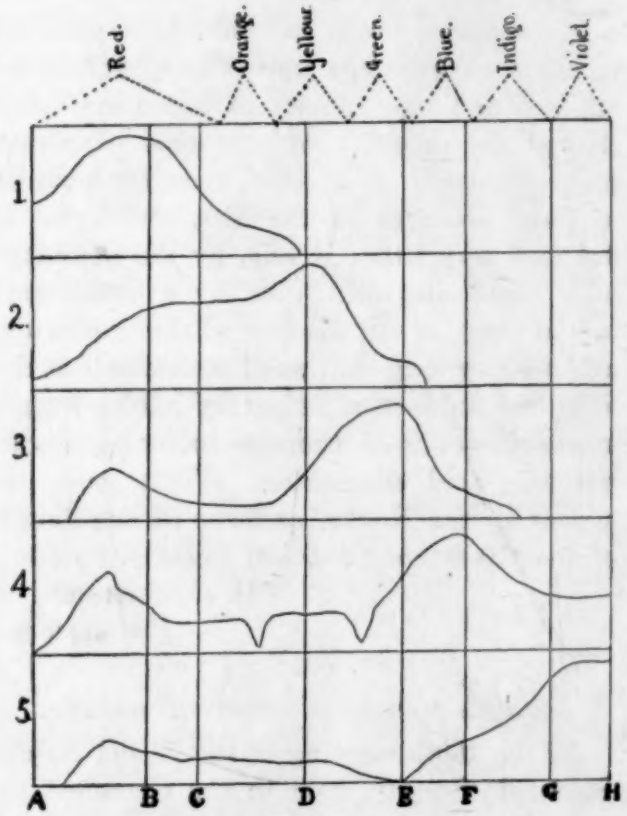

FIG. 1.

figure—1, red; 2, yellow; 3, green; 4, blue; 5, violet. The letters A, B, C, D, E, F, G, H are the ordinary significant points (Fraunhofer lines) of the solar spectrum. It will be seen that there is none of them, with the exception possibly of the red, which can be considered entirely a pure 'color.' However, they are fairly close approximations towards simple 'colors,' and as such are of some significance in regard to the subject under discussion.

In order to test the relative *phototropic*

* 'Die Heliotrop. Erschein. im Pflanzenreich.'

† *Ann. de Sci. Nat.*, IV.; 7; 1858. Both referred to by Sachs, 'Plant Physiol.,' p. 696.

‡ 'Lectures on Plant Physiol.,' p. 266.

effects of the screens mentioned, a number of metal frames were made so as to admit of free insertion of any of the plates into any of the four vertical sides of the frame. The plant to be used in the test was placed within the frame and enclosed on two opposite sides by opaque screens, and on the other two vertical sides were placed plates of two different 'colors.' On the top and the bottom were opaque plates. Then the plant was enclosed within the 'lantern' and placed equally distant from the two color-screens. No light was admitted to the plant excepting that coming through the two screens; and, since the top was covered, the plant was subjected to lateral illumination from two different 'colors' at the same time and from opposite directions. Care was taken to have only diffused daylight enter the screens and to have it equal in intensity. Now it seemed reasonable to conclude that if curvature of the stem of the plant took place toward *one* of the colored screens, the light which penetrated *that* screen produced most phototropic stimulus. The lanterns, not being actually air-tight, permitted the plant to live under more natural conditions of temperature, moisture and air than could be obtained by means of a double bell-jar.

The results obtained are summarized as follows and are represented by the curve given in Fig. 2, *I*. They rank in order named: *blue, white* (window glass), *violet, green, yellow, red, dark* (opaque). Between certain pairs of these screens the difference is not very great, but there is a positive difference in every case.

The main differences between these results and those of Wiesner, Guilleman and Sachs are in regard to the blue, the yellow and the red. Sachs states, p. 696, that curvature takes place behind a blue solution of ammoniacal oxide of copper as in full daylight. This is scarcely exact, because curvature is more prominent behind the blue screen than behind diffused daylight. It is also shown clearly here that curvature *does* take place behind red and behind yellow, though they produce less of a phototropic stimulus than any of the others, the yellow being stronger than the red. Wiesner states that no curvature takes place behind yellow, though he assigns some phototropic effect to red. An extra series was arranged to test the point as to whether no curvature takes place behind yellow or red. Lateral illumination was given through each of these two screens in different lanterns, and it was found that both produced distinct curvature. The lanterns were arranged so as to admit diffused skylight and also, at other times, weak diffused light from the room through the screens. In every case there was the same result.

Another series of experiments was performed with these colored screens to determine the decoloring effect of such light upon chlorophyll in solution. The solutions were

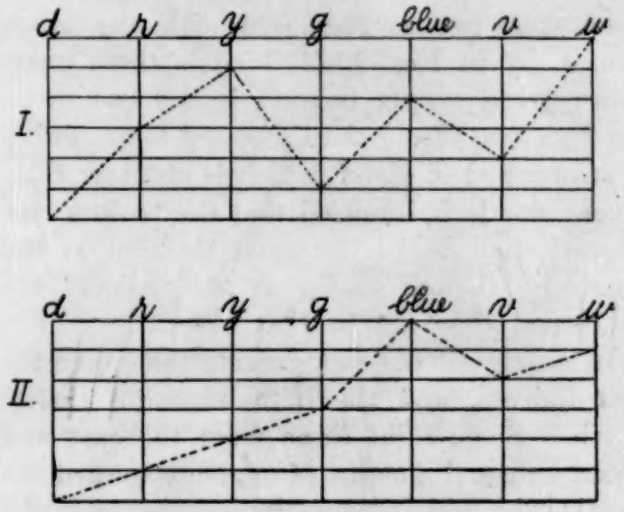

FIG. 2.

fresh alcoholic solutions and in each test the solutions were of exactly the same concentration, but solutions of different degrees of concentration were used to see if strength of solution had anything to do with the results. The conclusions reached were as summarized in Fig. 2, *I*, and in the following order commencing with the quality of light having the greatest decoloring effect: 1, *Diffused light* (in no case was direct sunlight used in the test); 2, *yellow;* 3, *blue;* 4, *red;* 5, *violet;* 6, *green;* 7, *darkness.* The result of this experiment showed that there was but small relationship between the phototropic effects and the decoloring effects upon chlorophyll in solution. It is quite clear that there is little in common (see Fig. 2, *I* and *II*). Sachs and Wiesner both say that the rays of low re-

frangibility produce greater decoloring effect than those of high refrangibility, but from the results obtained here it would seem that decoloration is in no way directly or inversely proportioned to refrangibility. It will be seen that the blue and the red are close together here, while in the solar spectrum they are far apart.

In the accompanying Fig. 2, *I* is given the curve of decoloration of an alcoholic solution of chlorophyll with the screens already described. The vertical lines represent *relative* quantity of effect—*d*, darkness; *r*, red; *y*, yellow; *g*, green; *v*, violet; *w*, weak diffused light. In Fig. 2, *II* is given the curve for relative phototropism. In both cases no attempt is made to represent the actual difference between any two as compared with any other two, *e. g.*, in Fig. 2, *II*, *blue* is three units above *green* simply because it happens to be stronger (in effect) than *diffused light*, which is stronger than *violet*, which is stronger than *green;* nor is it intended that the 'colors' indicated shall be in the exact position of the spectrum, though so far as the 'colors' are concerned they are in that order.

In nearly all the experiments the apparatus was indoors, and the light exposure chiefly north. Some light came from the east and about an equal amount of exposure toward the west. The first experiments were made in the greenhouse, but it was found that too much heat was produced, resulting in the wilting and even in the death of the plants. However, so far as carried on, the results were identical with those under diffused light.

The plants which proved most susceptible to phototropic influences were barley, wheat and tobacco seedlings. The best, most positive and the quickest results were obtained with wheat and with barley seedlings from five to forty mm. high. Other seedlings used were *Catalpa*, *Datura*, bean, pea, corn, sunflower and pumpkin.

No attempt is here made to deduce a physiological or a physical law from these phenomena because it is thought that sufficient data are not yet at hand; nor is there any quantitative effect estimated as existing between any two of the screens used. It is quite clear, however, that the statements of Sachs and others, namely, that the effect, whether phototropic or bleaching of chlorophyll solution, varies as the refrangibility, is not correct. It may be, however, that had the formulæ of their screens been given, it might be possible to see how they arrived at their results.

On looking at the spectra of the screens here given it may be seen that the blue permits considerable of other 'colors' to pass through, especially red. Now, since the phototropic effect of blue is greater than that of diffused daylight, the conclusion naturally follows that some portion of the solar spectrum is negatively phototropic because the blue as well as the other 'colors' passes through window glass. The question at once is suggested then as to where in the spectrum this negative portion is; but seeing that all the 'colors' here given are positively phototropic, the one conclusion is left, and that is but a mere suggestion, namely, that it may occur in those darker bands in the blue represented by the sharp down curve in its spectrum.

It is the intention of the writer to investigate this point by securing screens as nearly as possible corresponding to those portions of the spectrum; and at the same time to examine other intervening 'colors.'

J. B. Dandeno.

Agricultural College, Mich.

SCIENTIFIC NOTES AND NEWS.

At the last meeting of the Rumford Committee of the American Academy of Arts and Sciences, the sum of three hundred dollars was granted to Professor W. J. Humphreys, of the University of Virginia, in aid of his research on the shift of spectrum lines due to pressure; and the sum of two hundred and fifty dollars to Professor N. A. Kent, of Wabash College, in aid of his research on the circuit conditions influencing electric spark lines.

Dr. Carlos J. Findlay, of Havana, well known for his work on yellow fever, has been elected president of the American Public Health Association. The next meeting of the association will be held in Havana in April.

THE council of the Royal Meteorological Society has awarded the Symons gold medal to Hofrath Dr. Julius Hann, of Vienna, in recognition of the valuable work which he has done in connection with meteorological science. The medal, which is awarded biennially, was founded in memory of Mr. G. J. Symons, F.R.S., the originator of the British Rainfall Organization. It will be presented to Dr. Hann at the annual meeting of the society on January 20.

PROFESSOR FLINDERS PETRIE, F.R.S., has been appointed a delegate from the University of London to the International Congress of Archeology to be held at Athens in April, 1905.

The Botanical Gazette states that Dr. O. Melville Ball, of Batesville, Va., has been elected a member of the German Botanical Society.

DR. H. N. STOKES, of the U. S. Geological Survey, has been appointed chemist in the National Bureau of Standards. His temporary and mail address is Bureau of Chemistry, Department of Agriculture, Washington, D. C.

MR. W. J. PALMER, a graduate of the Ontario Agricultural College, has been appointed director of agriculture in the Orange River Colony at a salary of $6,000 per annum.

PRESIDENT H. S. PRITCHETT, of the Massachusetts Institute of Technology, has, according to the daily papers, tested the high speed electrical railway at Zossen, and has examined the Hamburg closed harbor. He will sail from Cherbourg for New York on November 11.

DR. ALBERT D. MEAD, professor of comparative anatomy in Brown University, has returned from a three months' trip through England, Holland, Germany, Italy and France, during which a thorough inspection was made of all the important biological laboratories and experiment stations.

MR. W. T. SWINGLE has returned to Washington after a study of the plants in the regions about the Mediterranean.

PROFESSOR FILIBERT ROTH, who holds the chair of forestry in the University of Michigan, has been elected forest warden of the state.

The American Geologist notes appointments as follows: Professor E. C. Perisho, of the State Normal School of Platteville, Wis., has been appointed state geologist of South Dakota and professor of geology in the State University. Dr. A. G. Leonard, assistant state geologist of Iowa, has been appointed professor of geology in the University of North Dakota, and Dr. A. F. Wilder, lately state geologist of North Dakota, has accepted the professorship of geology in the State University of Iowa.

H. CHESTER CROUCH, professor of mechanical engineering at the University of Colorado, died on October 29, in Boulder, of typhoid fever at the age of thirty-two.

THE death is announced of Dr. Wilhelm Rimpau, of Schlanstadt, Germany, known for his work on plant breeding.

THE winter meeting of the American Chemical Society will be held at St. Louis, Mo., on December 28 and 29, in connection with the meeting of the American Association for the Advancement of Science.

THE Association of the American Agricultural Colleges and Experiment Stations will meet at Washington on November 17, 18, 19 and 20. The Association of Official Agricultural Chemists will meet at the same place on November 19 to 21.

A CONFERENCE on secondary education was held last week at Northwestern University.

THE second International Congress of Comparative Religions will be held at Bâsle next year.

THROUGH the gift of Dr. Thomas Biddle, the Philadelphia Academy of Natural Sciences has acquired in Berlin a valuable collection of anthropoid apes.

THE British committee appointed to consider the alleged physical deterioration of the people has held its first meeting under the chairmanship of Mr. Almeric W. FitzRoy.

The Botanical Gazette states that Mr. Barbour Lathrop, of Chicago, who has made several expeditions at his own expense to different

parts of the world, in search of valuable seeds and plants for introduction into America, has returned. He has employed on his various expeditions Mr. D. G. Fairchild, who now resumes his connection with the U. S. Department of Agriculture as one of its explorers. The countries visited this year with a view to more thorough exploration later by the department are Italy, Sicily, Tripoli, Tunis, Malta, Egypt, German East Africa, Zanzibar, Portuguese East Africa, Natal, Transvaal, Cape Colony, Grand Canary, Madeira, Portugal, Spain, Bohemia, Sweden, Denmark, Holland, Belgium and England. Such seeds and plants as were secured were given by Mr. Lathrop to the Department of Agriculture for propagation and distribution, and it is hoped that some of them may prove of great value to the country, repaying him for his patriotic and generous interest in increasing the variety of food and ornamental plants of America.

UNIVERSITY AND EDUCATIONAL NEWS.

At Cambridge University 866 new students have been admitted, an increase of sixteen over last year. At the University of London the number is 1,016, an increase of 400 over last year.

The London *Times* states that in connection with the Liverpool Institute of Comparative Pathology (Liverpool University), of which Professors Boyce and Sherrington are directors, a tropical veterinary department has been established. Its objects are to train veterinary and medical men in the tropical diseases of animals, to afford facilities for research in such diseases and organize expeditions, and to organize preventive measures in the tropics against diseases of animals. A memorandum on the subject has just been issued by the institute. It is pointed out that the advantages which Liverpool possesses for the study of tropical medicine are equally applicable to tropical veterinary medicine, there being an immense foreign cattle trade with the port. The Johnston Laboratory of Liverpool University, opened last May by Mr. Walter Long, M.P., contains the fully-equipped laboratories of the Institute of Comparative Pathology, of the Tropical School of Bio-Chemistry and of the Cancer Research Committee, and is directly connected with the departments of bacteriology, pathology and physiology. These subjects, closely associated in the new Thompson Yates and Johnston Laboratories, it is pointed out, mutually help one another, and thus increase the thoroughness of training and greatly promote the opportunities for research. The secretary of state for war has approved of the course laid down by the institute, and in future officers of the Army Veterinary Department will be sent to Liverpool for their special training, the Army Department paying the fees. The Liverpool School of Tropical Medicine also trains officers sent specially by the government. In connection with the new department it is desired to establish a practical post-graduate class in veterinary medicine and also a school of veterinary medicine. Firms interested in the cattle trade have subscribed a considerable sum towards the expenses involved, but further funds are needed.

Lord Goschen has been elected chancellor of Oxford University in succession to the late Lord Salisbury. Lord Strathcona, Canadian High Commissioner, has accepted the nomination to the chancellorship of Aberdeen University.

Mr. Henry Sanger Snow has resigned the presidency of the Brooklyn Polytechnic Institute.

At Williams College, Mr. Lorande Loss Woodruff, A.B. (Columbia, 1901), A.M., 1902, has been appointed assistant in biology.

Arthur E. Wade, Cornell College, 1901, has been promoted to the head of the Chemical Department of the Medical College of Sioux City, Iowa.

H. J. Turner, Ph.D. (Johns Hopkins), has been appointed instructor in chemistry at Tufts College.

Dr. E. Grimmich, professor of anatomy, has been elected rector of the German University of Prague.

Dr. W. Wirtinger, of Innsbrück, has been called to a chair of mathematics at Vienna.